Comments on Issues Regarding the Assimilation of Cloud and Precipitation Data

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1. Introduction

A recently published article by Errico et al. (2007, hereafter EBM) attempts to review issues involved in the assimilation of cloud and precipitation observations in atmospheric data analyses. The review is properly motivated by a desire to bring attention to obstacles in resolving two important problems in atmospheric data analyses. These are: 1) Utilizing satellite observations that are currently discarded in operational atmospheric data assimilation systems because of high sensitivity to clouds and precipitation, and 2) estimating the hydrologic state of the atmosphere (i.e., hydrometeors) together with standard atmospheric fields of temperature, humidity, pressure, and wind. Despite the appropriate motivation, and for most part adequate identification of main issues, the review has two important shortcomings. First, the review is inaccurate and incomplete when referring to past published research results. Second, some conclusions and recommendations are contradictory to each other. In the following comments these shortcomings are addressed.

2. Review of past research

a. Inaccurate critical review

In the review of past research in section 2, the authors refer to early studies of assimilation of precipitation observations by variational data assimilation techniques as having "serious design flaws." Specifically the following studies are criticized: Zou et al. (1993), Županski and Mesinger (1995), Tsuyuki (1996), Zou and Kuo (1996), Kuo et al. (1997), Zhu and Navon (1999), Guo et al. (2000), and Xiao et al. (2000). Critical remarks relating to the above research include the following: "no consideration of background error correlations," "no background term altogether," "no consideration of dynamic balance," "treatment of observations as near perfect," "neglect of large error of representativeness or of observations operator errors," "misrepresentation of size of terms," "lack of convergence of solutions," and "consideration of only single cases."

The first major problem with this critical review is that the referenced studies are referred to as if they all treat the same problem while they are in fact different in subject and approach from each other. Rather elementary professional ethics imply that when a published peer-reviewed research study is criticized as having "serious flaws" it is the critics' responsibility to specifically explain what particular "flaw" is being identified in the particular study to make the criticism credible and useful. This criterion is not satisfied in EBM.

The second major problem with the critical review is that the listed examples of serious design flaws are incorrectly identified as the "design flaws" to begin with. For example, not using prior information in the formulation of data assimilation problems (i.e., no background term altogether) or not having prior error correlations (i.e., no consideration of background error correlations) are theoretically allowable assumptions when solving a least squares problem, which is the type of problem being solved by the variational data assimilation techniques (Tarantola 2005). The data assimilation solutions under these assumptions are possible and

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should be simply interpreted as the solutions given the assumptions. Besides, it is well known that the least squares approach to solving data assimilation problems already contains the fundamental assumption that the prior information as well as the information provided by model and observations are represented by stochastic quantities that have Gaussian statistical distributions (Tarantola 2005). Thus, unless this assumption is correct, which is not guaranteed when nonlinear models and observation operators are used, the least squares formulation of the data assimilation problem is potentially fundamentally incorrect. This implies that other published studies that are referenced in EBM and that use the least squares formulation may also qualify as having serious design flaws if the qualification is based on the use of incorrect assumptions.

The other serious design flaws, which are mentioned in the review such as treatment of observations as nearly perfect, no consideration of dynamic balance, and neglect of large errors of representativeness also belong in the category of possible assumptions or approximations when solving the least squares data assimilation problems and do not necessarily disqualify the research results. The results under the given assumptions should be interpreted, similarly to the cases with missing prior information, as the results that are relevant to the assumptions. This conclusion does not imply that the data assimilation results with the given assumptions are necessarily good. That would depend on the choice of the "goodness" criteria, but the results are certainly not seriously flawed. The remaining problems that are mentioned in the review under the design flaws category, misrepresentation of sizes of terms and lack of convergence of solutions, are hard to qualify because it is not clear what these comments actually refer to.

A more informative and potentially more useful approach to critically reviewing the past research would be to provide an analysis of the results' scientific credibility. The credibility, however, does not have to depend on the validity of underlying assumptions and/or approximations. Only when it is possible to demonstrate that noncredible results are the direct consequence of those assumptions used in the study approach, would it be justifiable to qualify the study as having serious design flaws. This approach to evaluating the credibility of modeling results in atmospheric sciences, including data assimilation, is rather basic. If this were not the case, many published modeling studies could be considered as having serious design flaws because most are produced by using (out of necessity) some assumptions and/or approximations that are not generally valid.

Perhaps two basic criteria for the scientific credibility of data assimilation studies would be that results are physically meaningful and that new knowledge is gained by them. The variational data assimilation studies criticized by EBM show physically meaningful results and provide new knowledge about the data assimilation techniques and impact of observations. Thus, they are credible and should not be characterized as having serious design flaws.

An additional objection to EBM's discussion regarding the reliability of the referenced published studies, is that the EBM authors imply that results from these are not useful because their "relevance to operations (is) questionable" (section 2). Besides, by not specifically defining "operations," it is not possible to understand the particular criterion for usefulness. The major problem with this criticism is that the research results that are published in scientific journals are evaluated based on operational usefulness. If "operational" usefulness were the main criterion for publishing research results in the atmospheric sciences, the community would be deprived of the majority of study results because most research is not conducted for the purpose of transitioning it to "operations."

The majority of data assimilation research is fundamentally motivated by the desire to investigate new ways to enhance the ability to quantify properties of the atmosphere, or more generally the natural environment. This is achieved by integrating knowledge and information acquired through modeling and observations. Such motivation is fundamentally scientific. As is the case with all other scientific studies, the ultimate goal of data assimilation research is to provide new knowledge that would benefit society. The insight gained from that research indeed contributes to weather and environmental prediction services but for the most part not directly.

b. *Incomplete review of past research*

The EBM article also attempts to present the progression of research over the past decade or so in precipitation and cloud data assimilation. It neglects to mention, however, most of the research that was done outside the operational weather analysis and forecast centers, with the exception of the publications by the first author (sections 2, 3, and 4). All of the studies mentioned in the article are important contributions in the progression of data assimilation research and should be emphasized. But there are many more published research studies that are omitted in the review, which are in fact complementary contributions and should be included. Moreover, the authors discuss the need to address such issues as

1) "complementary (to precipitation) use of cloud observations may be necessary" (section 2);
2) "from improved initial clouds, a more realistic description of the three-dimensional structure of the diabatic heating produced by condensation, known to interact with the dynamics, will result. This potential is greatest when assimilation is continuous within a significant time span, as in four-dimensional variational techniques, since temporal changes in the observed cloud field could be very informative" (section 2);
3) ". . . other models may be necessary, including radiative transfer models relating cloud and precipitation fields to observed radiances or reflectivities" (introduction in section 4); and
4) use of high-resolution models with more explicit microphysics (section 4c).

These points are discussed as if they have not yet been addressed in past research, which is demonstrated by not including any references to the past research addressing those issues. The above listed and other issues that are relevant to cloud and precipitation data assimilation are in fact addressed in a number of published studies that are not mentioned in the article such as studies by Greenwald et al. (2002, 2004), Benedetti et al. (2003a,b), Vukicevic et al. (2004, 2006), Koyama et al. (2006), Sun and Wilson (2003), Wu et al. (2000), Sun and Crook (1998), to mention some. All of these studies include high-resolution forecast models with explicit bulk parameterizations of cloud and precipitation microphysical processes (relates to point 4) and they all provide an explanation of possible benefits and difficulties when using such models and the associated adjoint models. The positive impact of temporal changes of the observed cloud fields when using a four-dimensional variational data assimilation approach (relates to point 2) is demonstrated in Benedetti et al. (2003b) and Vukicevic et al. (2006). "Information content" of the satellite infrared observations in the presence of clouds relative to the temperature and humidity variables is discussed in Koyama et al. (2006) (relates to point 1). The observation operators for radar data that are sensitive to clouds are reviewed in Sun and Wilson (2003) (relates to point 3). Also, the utility of radar reflectivity in estimating microphysical quantities using variational techniques was studied in Wu et al. (2000) and Sun and Crook (1998). Each of these and other similar studies that are done outside research groups that directly support operational data assimilation are valuable contributions to cloud data assimilation research and should be recognized as such.

The reference to Greenwald et al. (2002) is included in the article but incorrectly in section 4a. This study does not address the accuracy of a radiative transfer model at microwave frequencies as implied in the article. Instead, it presents an analysis of the radiative transfer model and the associated adjoint model for visible, near-infrared, and infrared wavelengths, which correspond to window channels on the Imager Geostationary Operational Environmental Satellites.

3. Discrepancies between recommendations

The discussion in section 4 in EBM implies that one of the main difficulties in cloud and precipitation data assimilation is associated with the nonlinearity of models that are involved in the assimilation. Some of the conclusions and recommendations from that discussion are contradictory to recommendations regarding the utility of an adjoint sensitivity analysis that are presented in section 5b. For example, the conclusions such as "Even simple nonlinearity can create multimodal analysis error distributions or corresponding cost functions," "The effect of nonlinearities can be difficult to characterize since they depend not only on structures of errors but also on amplitudes," and the associated recommendation: "These nonlinearities should be carefully investigated for individual as well as combinations of processes if their effects are to be understood" contradict with the recommendations regarding the linear sensitivity analysis, which include the following statements: "Adjoint sensitivity studies are also insightful for determining which fields may require better analysis for forecasting particular forecast fields. Errico et al. (2003) and Mahfouf and Bilodeau (2007) show that accurately analyzing both temperature and moisture fields is critical for forecasting precipitation." The apparent discrepancy is in promoting the linear predictability analysis while warning that the nonlinearities in the same models may cause difficulties in cloud and precipitation assimilation problems. Also, there is a discrepancy between emphasizing the critical role of model errors in nonlinear predictability studies by ensemble methods (section 5a) and not including the equivalent discussion in the context of an adjoint sensitivity analysis.

Instead of just promoting the adjoint sensitivity analysis that has already been well demonstrated in many past studies when the linear assumption is valid, a potentially more effective set of recommendations pertinent to the identified critical issues should emphasize the need for new sensitivity studies. These would help to identify ways to extend the range of validity of a linear assumption with the available nonlinear models. As is well known, the linear range depends not only on the model properties but also on the choice of control variables and diagnostic function (i.e., the forecast aspect and/or observables). Both the control variables and diagnostic functions include the choice of temporal and spatial scales. Thus, the linearization analysis of different observables and temporal and spatial scales and alternative control variables and even modeling approaches (e.g., bulk explicit microphysics) may be considered in future adjoint sensitivity studies. Such new studies could be done effectively outside complex, operational data assimilation systems, which is one of the recommended research strategies by EBM (section 7). Research regarding the effects of model errors in adjoint sensitivity analyses has, to the best of our knowledge, not yet been addressed in published literature. Perhaps, the sensitivity analysis should include adjoint solutions for an ensemble of different reference states that would be associated with different physical parameterizations.

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